XIII. *Illustrations of the Viscous Theory of Glacier Motion.*
Part II. *An attempt to establish by observation the Plasticity of Glacier Ice.*
By James D. Forbes, *Esq., F.R.S.S. L. and E., Corresponding Member of the Institute*
of France, and Professor of Natural Philosophy in the University of Edinburgh.

§ 3. De Saussure's *Theory.*
§ 4. *Modifications of* De Saussure's *Theory.*
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§ 3. De Saussure's *Theory.*

WHEN Gruner proposed the explanation of glacier motion by the sliding of the ice over its bed, and De Saussure illustrated and confirmed it by considerations drawn from the lubricating action of the earth's heat melting the ice in contact with the soil*, there is no reason to suppose that either of them thought it necessary to take into account the varying form of the channel through which the glacier had to pass, and the consequently invincible barrier presented to the passage of a rigid cake of ice through a strait or narrow aperture when it occurred. This is the more remarkable, because he conceives that the *inequalities of the bed or bottom* may be overcome by the hydrostatic pressure of the water, which he supposes may be imprisoned between the rock and the ice, so as absolutely to heave the latter over the resisting obstacles.

I believe that in no part of De Saussure's writings will there be found any, the slightest reference to the possibility of the glacier when fairly formed *moulding* itself to the inequalities of the surfaces over which gravity urges it; nor is there any trace of the correlative fact of an unequal motion of the sides and centre of the ice, which may in some sense be considered as the geometrical statement of the preceding physical fact. The fact of plasticity was suspected by Basil Hall, and more distinctly announced by Rendu, as shown in the first part of this paper; but it could not be proved until the geometrical fact of the swifter motion of the centre of the glacier relatively to the sides was established in 1842†. The contrary opinion at that time

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* To do Gruner justice, he appears to have been aware of the effects of the earth's heat and the lubricating action of the water thawed from the glacier: "Lorsque les côtés de l'amas [de glace] qui touchent la montagne, fondent en entier, toute la masse entraînée par son poids glisse sur son fond et s'avance dans la vallée," French translation, p. 333 . . . "il est vraisemblable que leur surface inférieure [*i. e.* des glaciers] se liquéfie autant, et peut-être plus que la supérieure," ib. p. 289.

† Edinburgh Philosophical Journal, October 1842, and Travels in the Alps of Savoy, p. 134.

generally entertained would have been conclusive against the hypothesis of plasticity called forth by the gravity of the mass.

So far, then, as appears from his writings, DE SAUSSURE considered the ice of glaciers to constitute a mass possessing rigidity in the highest degree, such rigidity in short as common experience assigns to ice tranquilly frozen in small masses, which is sensibly inflexible. It is in this sense in which I have spoken of DE SAUSSURE's sliding theory, as one which "supposes the mass of the glacier to be a rigid one sliding over its trough or bed in the manner of solid bodies*," and I adhere to the definition as excluding the introduction of the smallest flexibility or plasticity, to which the term rigidity is correctly opposed. I consider too that DE SAUSSURE's theory supposes the mass of the glacier to slide over its trough or bed in the manner of *solid* bodies, that is, not as a heap of rubbish or absolute fragments, such as a glacier sometimes precipitates over a rock, but which evidently did not enter into DE SAUSSURE's explanation, nor, in fact, required any theory.

As to the crevasses which form so prominent a feature of many glaciers (although many are in parts almost devoid of them), I do not recollect that DE SAUSSURE alludes to them as *facilitating* in any way the movement of the glacier, but simply as *results* of its motion and of the rigid character of ice. And I believe that this view (whether it was held by DE SAUSSURE or not) is substantially correct. The regular system of crevasses of a glacier is approximately transverse, rather arched upwards towards the origin of the glacier, and as DE SAUSSURE supposes the glacier to be pressed downwards by the mass of snow accumulating at its head, it is hard to believe that he could have regarded these fissures as in any way essential to its movement, even were they very numerous; the tendency of such a pressure from above would rather seem to be to pack the ice like an arch, opposing its convex side to the direction of the pressure.

The view now given of DE SAUSSURE's theory of glacier motion is not only conformable to what may be gathered from his writings, but expresses the unanimous understanding of his numerous commentators, followers and opponents. As some doubt has lately been hinted as to the definiteness of DE SAUSSURE's conception of a glacier as a mass devoid of flexibility and plasticity and urged down a slope *as a whole,* by the lubricating action of fusion in contact with the soil to an extent which, in extreme cases, might even give it the character of buoyancy, I will take the liberty of quoting some indisputable authorities amongst writers of name in different countries.

And first from DE SAUSSURE himself:—

" La chaleur de la terre fait fondre les neiges et les glaces, même pendant les froids les plus rigoureux lorsque leur épaisseur est assez grande pour préserver du froid exterieur les fonds sur lesquels elles réposent."—*Voyages*, § 532. *** " C'est elle qui entretient les torrents, qui, même pendant les plus grands froids, ne discontinuent jamais de sortir de tous les grands glaciers." § 533. ***

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* Travels, p. 362.

"Presque tous les glaciers reposent sur des fonds inclinés; et tous ceux d'une grandeur un peu considérable ont au-dessous d'eux, même en hiver, des courans d'eau qui coulent entre la glace et le fond qui la porte. On comprend donc que ces masses glacées, entraînée par la pente du fond sur lequel elles reposent, dégagées par les eaux de la liaison qu'elles pourraient contracter avec ce même fond, soulevées même quelquefois par ces eaux, doivent peu à peu glisser et déscendre en suivant la pente des vallées, ou des croupes qu'elles couvrent." § 535.

"Quand on considère que ces glaces reposent sur des plans inclinés, qu'il coule sous elles des torrens d'eaux qui les fondent par en bas, les détachent et les soulevent, ne sent-on pas que leur permanence dans la même place est une chose *physiquement impossible*?" § 2284.

RAMOND's account:

"La cause [de la marche des glaciers] est facile à concevoir: une masse qui pèse sur un plan incliné tend nécessairement à déscendre, et cette tendance est favorisée dans les glaciers par le choc des torrents qui roulent sous leur voûtes, par l'humidité que leur masse communique au terrain qui les porte, enfin par cette multitude innombrable de cavités qui creusent leur partie inférieure, et dont l'effet est de diminuer le frottement en diminuant l'étendue des surfaces*."

ELIE DE BEAUMONT's account of DE SAUSSURE's theory: speaking of the lava of Etna, he says, "L'écorce supérieure d'une coulée separée de l'écorce inférieure et du sol sousjacent par une certain épaisseur de lave liquide, ou du moins visqueuse, se trouve dans un état comparable à celui d'un glacier, qui, ne pouvant adhérer au sol sousjacent à cause de la fusion continuelle de la couche inférieure se trouve contraint à glisser†."

BISCHOFF's account:

"Das jährliche Vorrücken der Gletscher welches SAUSSURE ganz einfach aus einem allmäligen Herunterrutschen der unteren durch das Aufthauen des Eises schlüpfrig gewordenen Seite des Gletschers auf der schiefen Fläche des Bodens erklärt, ist eine bekannte Thatsache‡."

AGASSIZ's account:

"Autrefois on admettait tout simplement qu'ils glissaient sur leur fond, en vertu de leur propre pesanteur, et que ce glissement était favorisé par les eaux au fond de leur lit. C'était l'opinion de SAUSSURE§."

MARTINS' account:

"DE SAUSSURE, ESCHER DE LA LINTH, ANDRÉ DE LUC, attribuent cette progression au poids des glaces et à l'affaissement produit par la fonte de la face inférieure qui repose sur le sol ‖."

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* Voyages en Suisse par COXE, traduit par RAMOND, ii. 119, 1790.

† Mémoires pour servir à la Déscription Géologique de la France, iv. 177, published 1838.

‡ Wärmelehre, p. 180, published 1837. § Etudes sur les Glaciers, p. 152, published 1840.

‖ Sur les Glaciers de Spitzberg. Bibliothèque Universelle, 1840, tom. xxviii. p. 166.

STUDER's account :

"Die bisher fast allgemein herrschende Theorie erklärte die Bewegung der Gletscher aus der Schwere allein. Es soll die Gletschermasse als STARRER KÖRPER auf ihrer Felsgrundlage, wie auf einer schiefen Ebene, theils durch ihr eigenes Gewicht theils durch dem Druck der höheren Eis- und Firnmasse herunter gleiten (GRUNER, RAMOND, KUHN, DE SAUSSURE, ESCHER)*."

This last testimony of the most exact and most learned of the living Swiss geologists as to the sense in which DE SAUSSURE's theory has always been understood is so important that I shall add a translation : "The hitherto generally prevailing theory explains the movement of the glaciers by gravity alone. The glacier masses are considered as RIGID BODIES, which slide down over their rocky beds partly by their own weight, partly under the pressure of the higher ice and *névé*." My interpretation of the views of DE SAUSSURE as regards the rigidity of the glacier ice is thus borne out by an independent authority, for M. STUDER's work and my own appeared simultaneously. It is further confirmed by private communication with another eminent Swiss naturalist nearly connected by relationship with DE SAUSSURE himself, who is more intimately acquainted with the opinions and writings of his illustrious kinsman than any other person now alive, and who considers that DE SAUSSURE's views were confined to the general analogy of the glaciers to solid masses sliding down inclined planes, and that the effects of the inequalities of the channels and forms of the ice-basins were not comprehended in his theory.

If we feel surprise that a naturalist and observer so eminent had not adverted to the difficulty of imagining a solid cake of ice, even though perfectly detached from its bed, to disengage itself from the obstacles and sinuosities of its rocky channel, we should remember,—*first*, that the explanation is given in the most general terms, and there is no appearance that its author looked more closely at its consequences and details than to satisfy himself that a sliding motion in the abstract was rendered possible by the action of the earth's proper heat, an ingenious and philosophical element of the theory (however inadequate), and that which being due principally to DE SAUSSURE, renders the theory properly his, and connected it with his ingenious inquiry into this curious part of physics as a distinct and wholly independent investigation. *Secondly*. Every one knows how an application of a principle so true and so ingenious leads men of even the most exact habits of thought to overlook difficulties in a subject almost unstudied. DE SAUSSURE did much for our knowledge of glaciers, and he saw much which no one had observed before him : we must not blame him if, yielding to a true and natural analogy of sliding bodies, he overlooked real and great difficulties inherent in the conception of a glacier as a solid continuous mass and highly rigid. *Thirdly*. In DE SAUSSURE's time no plan or map, worthy of the name, of any glacier existed, and this was a blank which even DE SAUSSURE did not attempt to supply. The popular notion of a glacier, which it is certain he had in his mind

* Lehrbuch der physikalischen Geographie und Geologie, 1844.

when he penned the passages which relate to their motion, is a mass of ice of small depth and considerable but uniform breadth sliding down a uniform valley, or pouring from a narrow valley into a wider one, as is the case with a vast majority of glaciers tolerably accessible, and which alone were visited at the time of publication of the first edition of the *Voyages dans les Alpes*. In all these cases the lateral resistance might easily be overlooked, and the popular comparison to one solid body sliding on another and lubricated by its own liquefaction might be accepted as a complete explanation; as has even been done at a later period by those who have attempted to illustrate DE SAUSSURE's theory by experiment, but who, like him, neglected the form and undulations of the bed in which it rests.

§ 4. *Modifications of* DE SAUSSURE's *Theory.*

DE SAUSSURE and his immediate followers appear to have considered the crevasses which occur transversely in most glaciers, as the result of the inequalities of the beds down which they are constrained to move; but other writers have imagined that the part which these crevasses perform in the phenomena of glacier motion is fundamental, and essential to the existence of the movement at all. Some writers have remarked that the fall of ice blocks over the precipice which often occurs near the lower end of glaciers, leaving the superior portions unsupported, allows them to advance to fill the position formerly occupied by the portion of the now fallen ice. But in this case it would appear that cause and effect are in some degree confounded. The ice about to be projected over the cliff must either advance towards its fall by its own gravity, or by the pressure of the parts behind. If its own gravity suffices, the same cause will urge the ice behind it to move similarly, whether the block in question fall or not; and if it be the pressure from behind which shoves it on, then still more is the pressure of the entire glacier the cause of motion of the entire glacier, irrespective of the precipitation of its more advanced part.

Thus, M. MARTINS' theory of the progression of glaciers is, that the weight of the parts causes them to separate by fissures into wedge-shaped masses, without their sliding along the bottom; that the fissures become filled with frozen snow, and that thus the glacier is perpetuated and extended year by year. " Cette progression," he says, " n'est donc ni un glissement ni un affaissement difficiles à comprendre, puisque la glace doit adhérer au sol, mais un démembrement successif*." Besides other objections, it is now universally admitted that the glacier-proper does not grow by the consolidation of snow in its fissures.

But setting aside the attempt to render the sliding motion of the entire glacier considered as a plane slab more easy, by considering the motions of the parts instead of the motion of the whole, we are led to notice the attempt to reconcile the sliding theory to recent observation, by ascribing to the crevasses of the glacier the important office of enabling it to accommodate itself to the inequalities of its channel.

* MARTINS sur les Glaciers de Spitzberg et de la Suisse. Bibl. Univ. Juillet 1840,

Our object is, in this section, merely to state the view in its most plausible form, which in the succeeding section we shall controvert by experiments giving it a direct negative. In the third portion of this essay we shall enter more at large into the phenomena of crevasses, and mention other objections to this hypothesis and every modification of it.

According to this view, the friction of the ice against the sides of the valleys will produce a dislocation of the glacier into longitudinal stripes (as shown in Plate VII. fig. 1.*), where a transverse line bb' becomes by the irregular motions of the ice distorted into the zigzag form $hcc'h$. Or if we suppose the plasticity of the ice to be sensible, but that its action is accompanied with fractures, the abruptness of the angles of the figure will be softened, as in the broken line $lmm'l$ in the lower part of the same figure. This latter hypothesis evidently merges into the true plastic theory, when the part of the progression due to the flexure of the transverse lines bears a large proportion to the effect of the longitudinal slide, or more generally, when the surfaces of sliding or yielding become greatly multiplied, when the notched line will merge into a curve.

The passage of the glacier through a gorge or contraction is explained on the same view by figure 2, where the resistance of the sides having occasioned a series of parallel longitudinal rents as before, the portion of the glacier beyond the limits of breadth of the gorge BB' is supposed to be detained or embayed whilst the intermediate columns slip through.

§ 5. *Experiments at Chamouni on the Plasticity of Ice.*

It has been shown that in order to reconcile DE SAUSSURE's theory of sliding motion with the ascertained fact that the centre of the glacier moves faster than the sides, it had been assumed that solutions of continuity or longitudinal crevasses were formed parallel to the length of the glacier, by means of which the central portion slides past that adjacent to it, and so on for successive strips as we approach the sides, the more rapid retardation near the sides being rendered mechanically possible by the increased number of these longitudinal dislocations.

The result was therefore predicted to be that the glacier would be found to move by *echelons*, or that strips of ice of a certain number of feet, or yards, or fathoms, would move either suddenly or by gradual sliding, but at all events so as to mark by an abrupt separation at the longitudinal fissure, that the one portion of ice has slipped past the other by a distinct measurable quantity.

When I first learnt at Geneva, in August 1844, from Mr. HOPKINS's published papers†, that this was really the author's meaning, it occurred to me that the proof

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* These figures and their interpretation are taken from Mr. HOPKINS's First Memoir in the Cambridge Transactions, vol. viii. part 1. A figure similar to the first is to be found in a more recent paper by the same author in the Philosophical Magazine for June 1845.
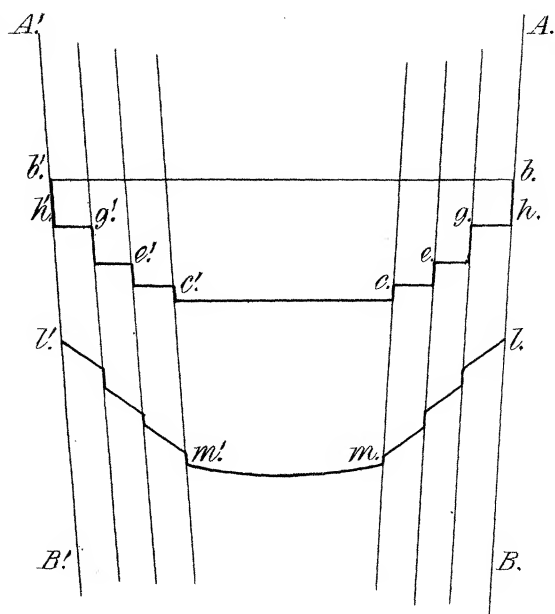
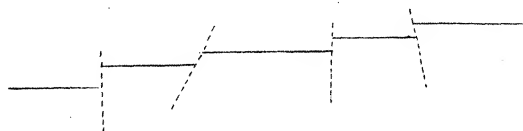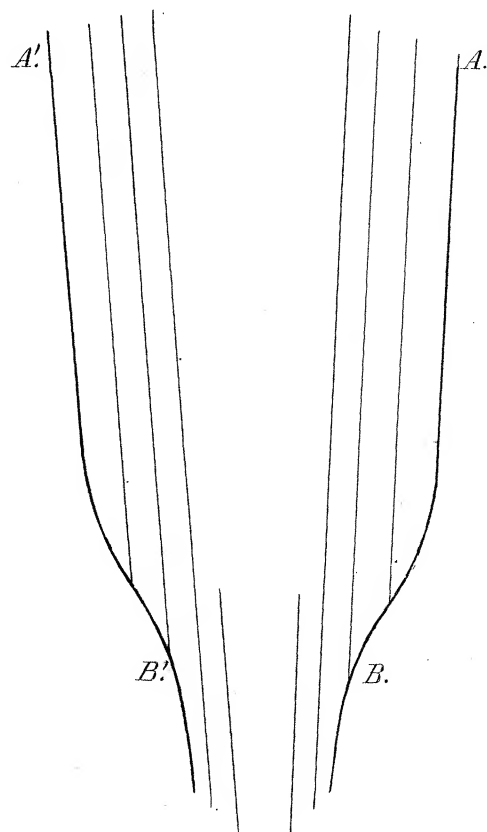† Cambridge Transactions, vol. viii.

Fig.1.*p.162.*

Fig.2.*p.162.*

Fig.3.*p.172.*

between the rival theories was easy, and that it was only necessary to place a series of marks in a right line transversely to the glacier, and observe whether they were displaced by an imperceptible flexure, or whether they slid past one another by sudden dislocations.

Such a proof was independent of any assertion as to the existence or not of such fissures as those contended for, about which different opinions might be formed, especially as they might be asserted to exist although invisible to the eye. Being satisfied in my own mind of the non-existence of such fissures wherever the ice is not violently dislocated and descends a steep place in a tumultuous manner (which, as already mentioned, is not the case which we consider), I had no hesitation in predicting that the result of the experiment would be confirmatory of my theory, and contradictory of the other; that the transverse line would be found to become a continuous curve, and that no other system of fissures could be found in the glacier satisfying the mechanical postulate of the greater velocity of the central parts of the glacier, than the *ribboned structure* of the ice, which I had already pointed out as resulting from a forced separation of the semi-rigid ice, at a vast, though finite, number of points in the breadth of the glacier, and which I showed to exist exactly in the direction required for releasing the mass from the tension induced by the gravity of its parts.

Having gone to Chamouni a few days later, I looked out for a place where the ice should be as compact as possible, wholly devoid of open fissures, and if possible continuous up to the bank. This latter condition I found it impossible to fulfil on the Mer de Glace, at least without ascending to the *névé*, which might be objected to as less rigid than the glacier proper. The former condition was well-satisfied in a sort of bay on the west side of the Mer de Glace between the Angle and Trelaporte, exactly under the little glacier of Charmoz. The part adjoining the western shore of the glacier is indeed highly crevassed, and therefore unfit for this experiment; but at the distance of fifty or sixty yards from the moraine it becomes remarkably flat and compact for a space of about seventy yards in width, and several hundred yards in length, throughout which space there is not a single open crevasse. Now this compact area of ice presents the veined structure in a nearly longitudinal direction, with a degree of delicacy and distinctness not to be found in any other part of this glacier (as I had already remarked in my *Travels,* p. 159), and it contains no other trace of a system of longitudinal fissures or lines of separation of any kind, which could render mechanically possible the distortion of this flat compact surface of so great an extent. Now I have always observed that the veined structure near the side of a glacier is best developed where the ice is least crevassed, or the continuity of the mass most perfect; a fact stated and referred to its true cause from the first date of my speculations on the origin of the blue veins, in the following words :—" The veined structure invariably tends to disappear when a glacier becomes so crevassed as to lose horizontal cohesion, as when it is divided into pyramidal masses. Now this immediately follows from our theory;

for as soon as lateral cohesion is destroyed, any determinate inequality of motion ceases, each mass moves singly, and the structure disappears very gradually*." Now the ice at the point in question is the compacted ice which has just passed round the great promontory of Trelaporte, having been rent by numberless chasms, and which is consolidated by pressure in the bay in question, whilst the centre of the glacier being still on the steep is deeply crevassed. The structure of the even ice is continuously striped with a regularity comparable to that of the finest chalcedony for a distance of some hundred feet. This structure must *have been produced on the spot,* since no such perfect structure exists higher up, and if it did, it must have retained all the marks of dislocation due to the formation and reconsolidation of the fissures, which are so numerous and wide as to render the passage of the glacier quite impracticable if we follow the same strip of ice up towards the promontory of Trelaporte. Let it then be recollected that the structure is *produced here,* under our eyes, on the very spot where the experiments about to be detailed were made, and that the structure in question produced a vertical slaty cleavage so distinct, that the ice broken into hand specimens may be split parallel to it like any slaty rock, and that the fine hard laminæ projecting vertically after the glacier has been washed by rain, permitted the blade of a knife to be thrust between them to a depth of several inches, although they are rarely more than a quarter of an inch thick.
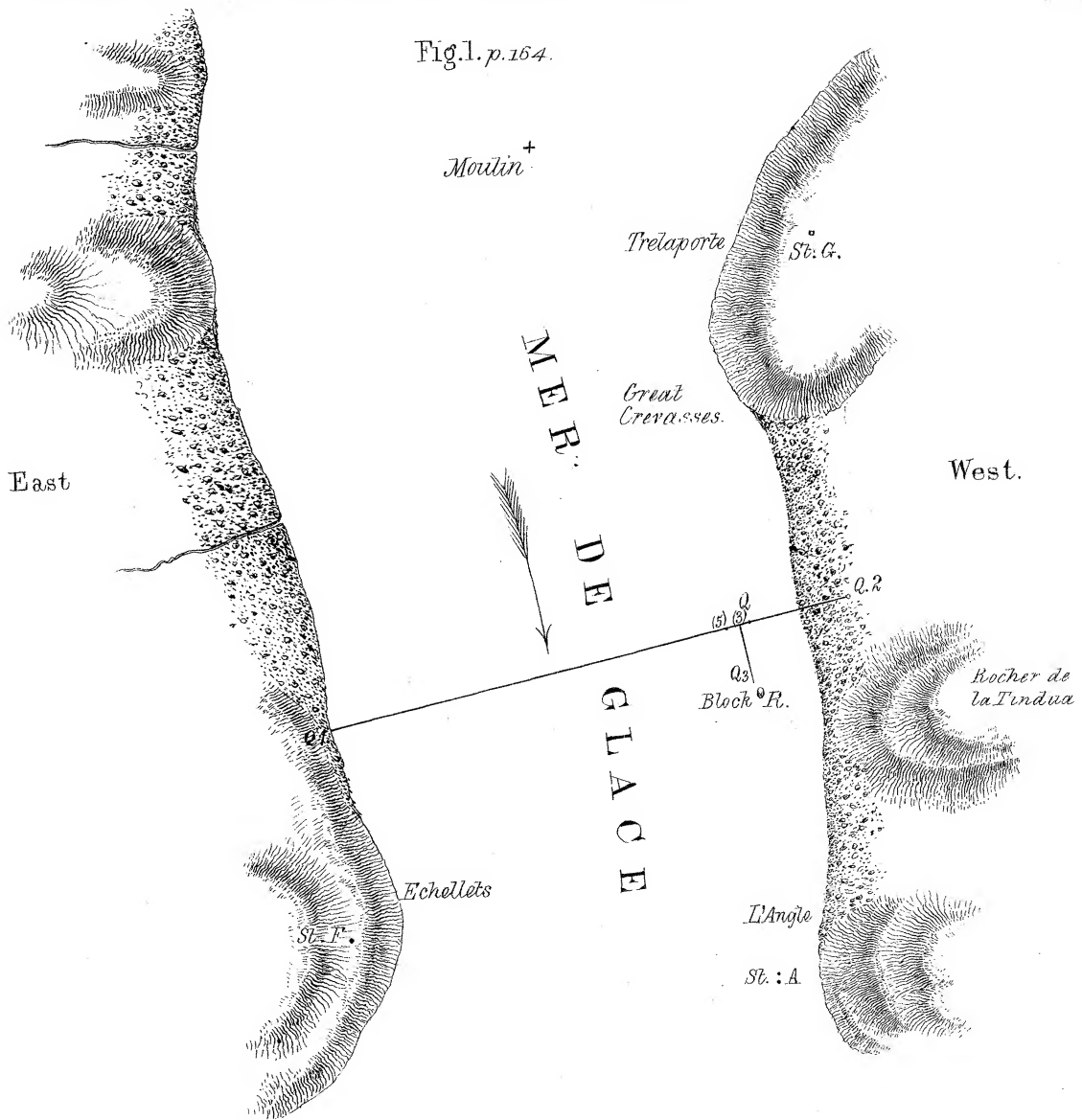
I shall now describe the actual measurements made upon the glacier in order that my method of proceeding in similar cases (when I have only published results) may be understood.

The general position of the experimental surface will be understood from the topographical sketch (Plate VIII. fig. 1.) The theodolite was planted at a fixed point on the ice Q, just within the crevassed portion, which intervened between it and the western shore of the glacier. This point of fundamental and constant reference was fixed by an exactly vertical hole pierced with an iron *jumper,* or blasting iron, one inch in diameter, and was frequently deepened in order to preserve the centre as exactly as possible in the same vertical line in the ice. The theodolite was centred over it at every observation by means of a plummet, which nearly filled the cylindrical hole and permitted an adjustment, which one day with another might be accurate to about one-tenth of an inch. No stick was placed in the hole, but when not in use it was covered by a large flat stone, which effectually prevents congelation in ordinary weather†. The adjustment of the theodolite on the ice is always a matter for patience, but I succeeded in rendering it perfectly stable when once erected by inserting the three feet in cavities in the ice, and filling them carefully with ice chips.

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* Third Letter on Glaciers, Edinburgh Philosophical Journal, October 1842, and Appendix to Travels, 1st edit. p. 407.
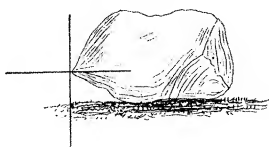
† On one occasion this precaution having been neglected (in the case of a different mark on the ice), the hole was found completely frozen up after exposure to a day or two of severe weather in the month of August. It was however recovered by observing the beautiful stellar form of the ice-crystallization.

Fig. 1. *p. 164.*

Moulin +

Trelaporte   St. G.

East

Great
Crevasses.

M E R.   D E

G L A C E

West.

Q
(5) (3)

Q.2

Q3

Block R.

Rocher de
la Tindua

Echellets

St. F.

L'Angle

St. : A.

| 0 | 1000 | 2000 | 3000 | 4000 feet |

Scale $\frac{1}{12,500}$ of Nature

Fig. 2. *p. 164.*

Fig. 3. *pp. 165.169.*

q

Q1 ......(5)............(4)......(3)......(2)......(1)......Q......Q2

2569 ft.   180 ft.   120 ft. 90 ft. 60 ft. 30 ft.   Q   550 ft.

Mark on les Echellets.

Q3

J. Basire lith.

The theodolite, placed at **Q**, was pointed with its vertical wire on the well-defined angle of an erratic block **Q1** on the opposite eastern bank of the glacier, above Les Echellets (see Plate VIII. fig. 2.). By causing the telescope to traverse in a vertical circle, a transverse line joining the points **Q, Q1** was determined, and several stations were fixed in the compact ice eastwards of **Q**, at distances from it of 30, 60, and 90 English feet and subsequently at 120 and 180 feet. These were numbered in succession (1), (2), (3), (4), (5), and the permanence of their positions in the ice was secured as before by carefully driving vertical holes two feet deep, which were occasionally deepened, and covered with flat stones when not in use. As these points were in succession nearer to the centre of the glacier, they were expected to move with gradually increasing velocity in advance of the imaginary line **Q, Q1** drawn across the ice.

But as the theodolite stationed on the glacier at **Q** must partake of its motion whilst the mark **Q1** on the bank remained at rest, the visual line **QQ1** would appear to revolve *towards* the origin of the glacier, and hence the relative advance of the points (1), (2), &c. would seem too rapid. To estimate the correction for this error the velocity of the glacier at **Q** must be determined, and also the distance **QQ1**. For the former purpose the following method was adopted. When an observation at station **Q** had been completed, by pointing the telescope on **Q1** and observing the apparent advance of the points (1), (2), &c., the telescope was reversed in the Y's, or turned 180° towards the western moraine, upon which it indicated from day to day a new position, owing to the angular revolution of the line joining the fixed point **Q1** and the moveable point **Q**. The point **Q2** in the topographical sketch (Plate VIII. fig. 1) indicates the point where the visual line touched the moraine at the commencement of the observations on the 9th of August 1844. By the application of a scale or a similar method, the apparent advance of **Q** referred to the moraine **Q2** was regularly measured. It is thus obvious that these apparent motions were *too great* (by the property of diverging lines) in the ratio of the distance **Q1.....Q2**, to **Q1.....Q**: and hence it became necessary to ascertain the position of **Q2** as well as **Q1**. For this purpose a base-line of 300 feet was measured on the ice parallel to the length of the glacier, or perpendicular to the transverse visual line, extending from the point marked (3) to the point **Q3** in fig. 3, whence by the theodolite the following bearings were taken :—

$$
\begin{array}{lcc}
\text{Q1} \ . \ . \ . \ . \ . \ . & \overset{\circ}{0} & \overset{\prime}{0} \\
\text{(3)} \ . \ . \ . \ . \ . & 83 & 6 \\
\text{Q2} \ . \ . \ . \ . \ . & 148 & 0
\end{array}
$$

From which we deduce the distance from

$$
\begin{array}{lcc}
\text{Q1 to (3)} & . \ . \ . & 2479 \text{ feet.} \\
\text{(3) to Q2} & . \ . \ . & 640 \text{ feet.}
\end{array}
$$

But as (3) is 90 feet east of **Q**, we have

$$\text{Q.....Q1} = 2569 \text{ feet.} \qquad \text{Q.....Q2} = 550 \text{ feet.}$$

The apparent motion of Q measured on the moraine is greater than the true motion in the ratio $\frac{2569+550}{2569}$ or $\frac{6}{5}$ nearly. The actual motion of Q is readily deduced as well as the apparent rotation of the visual line QQ1. Thus during 16·75 days, the duration of the experiment, the apparent advance of Q referred to the moraine Q2 was twenty-three feet six inches, which at the distance Q1.....Q2 (3119 feet) subtends an angle of 25′ 54″, or almost exactly *one and a half minutes daily*.

The motion of Q during the interval of any two observations of the marks (1), (2), (3), &c. being thus known, the correction applicable to the apparent advancement of the said marks beyond the visual line is at once found by the proportion

$$Q.....Q1 : Q\text{'s motion} :: Q.....(1) : E,$$

where E is the error of apparent position of mark (1). Thus, suppose the apparent motion on the moraine Q2 to be seventeen inches; this, reduced in the ratio of 6:5, gives 14·2 inches for the progress of Q. If the effect on the apparent place of a mark ninety feet from Q were required, we should have 2569 : 14·2 :: 90 : 0·50 inch.

I shall first detail the observations on the total motion of the glacier at Q, during the period to which the experiment extends, with the corrected daily motion and a memorandum of the state of the intervening weather, which accounts by its excessive variability for the remarkable variation of the progress of the glacier[*].

<center>TABLE I.</center>

Date.	Interval.	Apparent motion from commencement.	Corrected daily rate.	Weather.
	days.	ft. inch.	inch.	
August 9. 6 P.M.	0	0 0		9. Fine.
10. 3½ P.M.	0·90	1 5	15·6	10. Some rain. 11. Some rain.
12. 2 P.M.	1·94	4 0	13·2	12. Some rain. 13. Snow storm. 14. Some rain. 15. Snow storm. 16. Wet.
17. 2½ P.M.	5·02	11 6½	14·8 {	17. Fine ; melting snow on glacier. 18. Showery. 19. Fine ; snow still on glacier. 20. Very fine. 21. Very fine.
20. 1 P.M.	2·94	16 0	15·0	22. Rain.
23. 2 P.M.	3·04	19 10	12·5	23. Showery. 24. Cold rain. 25. Fine.
26. Noon.	2·92	23 6	11·8	26. Fine. Glacier dry.

The first three marks on the ice, those placed thirty, sixty, and ninety feet nearer the centre of the glacier than Q, were fixed on the 9th of August, the mark (4) at 120 feet was planted on the 17th, and the mark (5) at 180 feet, on the 19th. The following are the observations on the *apparent* motions of these points past the transversal line through Q, as well as these relative motions corrected for the real movement of the station Q, as explained in last page. To avoid an illusory appearance of accuracy, the results are given to the nearest twentieths of an inch, which is below the possible errors of observation.

[*] See Travels in the Alps of Savoy, &c., p. 148.

TABLE II.

Table of the *Apparent* and *True* Motions of the Stations *relatively to Station Q.*

Date.	Interval.	Motion of Q.	Apparent relative motions.					True relative motions.				
			30 feet.	60 feet.	90 feet.	120 feet.	180 feet.	30 feet.	60 feet.	90 feet.	120 feet.	180 feet.
	days.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.	inch.
August 9. 6 P.M.	0·0	0·0	0·0	0·0	0·0			0·0	0·0	0·0		
10. 3½ P.M.	0·90	14·0	0·8	1·9	2·3			0·65	1·6	1·8		
12. 2 P.M.	1·94	25·6	1·7	3·1	3·7			1·4	2·5	2·8		
14. 1 P.M.	1·96	29·0	1·5	2·8	4·3	0·0		1·15	2·1	3·25	0·0	
17. Noon............	2·96	44	2·1	4·0	6·0	7·1	0·0	1·6	2·95	4·45	5·0	0·0
19. 5 P.M.	2·21	33·2	1·7	2·7	4·4	6·2	8·5	1·4	2·1	3·25	4·75	6·15
23. 1 P.M.	3·83	48	2·5	5·8	7·7	10·3	15·0	1·95	4·7	6·05	8·15	11·75
26. Noon............	2·96	34·6	2·1	4·6	6·5	8·0	11·0	1·7	3·6	5·3	6·4	8·6
Motion from 9th to 26th...	16·76	228·4*						9·85	19·55	26·9		
Motion from 17th to 26th...	11·96	159·8						6·65	13·35	19·05	24·3	
Motion from 19th to 26th...	9·00	115·8						5·05	10·4	14·6	19·3	26·5

The results in the preceding Table have been divided into three periods, corresponding to the unequal times of observation of the last two and the first three stations. The first line of addition includes the motion at thirty, sixty, and ninety feet for nearly seventeen days; the second sum contains the comparative results for four stations throughout twelve days, and the last line contains the entire relative motions of five stations for nine days. These results may be further analysed, as in the following Table, which exhibits the mean daily motion in inches corresponding to each point for these distinct periods, and also the ratio of the relative motion of each point to the actual motion of the glacier at Q, or the zero point, during the same interval.

TABLE III.

Interval in days.	Actual motion of Q, or zero point.	(1.) 30 feet.		(2.) 60 feet.		(3.) 90 feet.		(4.) 120 feet.		(5.) 180 feet.	
		Mean daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.	Daily relative motion.	Ratio to actual motion.
	inch.	inch.		inch.		inch.		inch.		inch.	
16·76	228·4	0·59	0·043	1·17	0·086	1·60	0·118				
11·96	159·8	0·56	0·042	1·12	0·084	1·59	0·119	2·03	0·152		
9·00	115·8	0·56	0·044	1·16	0·090	1·62	0·126	2·16	0·167	2·94	0·229

This Table shows, first, in a striking point of view, the regularity of action of the law by which the variable motion of the different transversal points in the glacier is governed, since the movement in the different intervals bears so near a proportion, that when estimated in terms of the actual motion of the glacier at the place, the relative motion of the parts scarcely differs by unity in the second place of decimals, and is generally much under it. Taking into account the inevitable errors of observation and the extraordinarily unfavourable circumstances of the weather, it is in the very highest degree improbable that this *law of continuity* of the *partial* motions can be ac-

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* The true sum ought to be about four inches greater. The difference arises from the impossibility of estimating the correct velocities for the fractional intervals.

counted for by any casual justling or sliding of one finite portion of the ice past another, which would inevitably have left some of the points relatively at rest during some one of the many intervals of observation, and given to others evidence of a starting motion until friction had established a fresh position of repose amongst the struggling masses.

Secondly. This Table enables us to establish not only the continuity of motion of any one point, but the continuity of the relation which connects the points (1), (2), (3), &c.   For instance, the relative motions of (1) being

$$·59 \qquad ·56 \qquad ·56,$$

and those of (2) being

$$1·17 \qquad 1·12 \qquad 1·16,$$

the ratios are

$$1·98 \qquad 2·00 \qquad 2·07.$$

In like manner the ratios between (3) and (2) will be found to be

$$1·37 \qquad 1·42 \qquad 1·44.$$

Thirdly. The flexure of the ice may be conveniently represented by a diagram, in which the several ordinates are set off corresponding to the relative spaces moved over.   But to find the initial positions of the fourth and fifth marks, the proportional motion for the first period, when they were not observed, must be deduced from the comparative velocity of the period when the observations were comparable.   Thus by Table II. the relative velocity of (3) to (4) during the time that they were observed together is $19·05 : 24·3$; consequently whilst (3) moved over $26·9$ inches (4) would have moved over $34·3$ inches; the proportional motion for $16·75$ days.   In like manner for the mark (5) we have the simultaneous motions of (3) and (5) expressed by $14·6$ inches and $26·5$ inches, and hence by proportion, as before, we find

$$14·6 : 26·5 = 26·9 : 48·8 \text{ inches,}$$

the relative motion of (5) in $16·75$ days.

From these data the simultaneous relative motions of these six stations may be projected in a curve, or rather polygon, as shown in Plate IX. fig. 1.   This is interesting, as showing very plainly, not only the regulated increase of swiftness of the glacier towards the centre, but that the *variation of the variation* is clearly brought out, indicated by a convexity in the direction of the motion, and confirming the general principle long ago announced by me, that the retardation is relatively greatest towards the side and less towards the centre.   I appeal to any one conversant with the laws of mechanics in their practical application, whether the manifest continuity of such a law does not plainly include a continuity in the mutual action of the parts of the mass under experiment, and even independent of the manifest absence of great dislocations, would not establish the doctrine of a molecular yielding, or plasticity in the ice as opposed to the irregular justling of great blocks, admitting that such could exist unperceived.

The period through which this experiment extended (seventeen days) is conclusive against the idea that a small flexure could take place until the accumulated strain on the solid produced a rupture, which relieved the strain, and so forth, *per saltum*. The continuity of glacier motion in every case except that of precipitous descents or ice-falls, first proved by my experiments in 1842, is now universally admitted by those who have had any personal experience in the measurement of glacier motion, however opposed to my theoretical views *. The changes for seventeen days were connected (as has been shown) by a law of continuity established by numerous intervening observations; and the flexure or distortion of the ice amounted in this time to no less than *four feet* at the opposite ends of a line 180 feet in length. It is quite certain, from my own previous observations and those since made by M. AGASSIZ's directions on the glacier of the Aar, that the movement thus shown to have continued seventeen days without a *saltus* would have continued the whole season in the same manner. In fact, the *deformation* or flexure thus observed being sufficient to account for the whole excess of the central above the lateral motion, is in itself an explanation, and a proof that the explanation is adequate, and leaves nothing residual to be accounted for by *saltus*†.

I have more to add on this subject, but shall first give an account of an extension of this experiment on the actual flexure of the ice, upon so elaborate a scale as I scarcely ventured to hope would prove successful, especially as the time I could devote to watch its progress was small, and the circumstances of weather excessively unfavourable.

Having succeeded so well with the thirty feet station in the transverse line, I thought of multiplying the points of observation still further, so as to obtain a polygon of flexure more nearly approaching to a curve. This I did by making the first ninety feet of the transverse line, *i. e.* the space between Q and (3), Plate VIII. fig. 3, the subject of more immediate experiment, fixing in it forty-five stations only two feet apart. After several partial failures, which gave me, notwithstanding, encouraging results, I selected this plan. A space a foot wide and ninety feet long was cleared with hatchets and ice tools, so as to arrive at a nearly even surface of the hard delicately veined ice; and gutters were made so as to drain as far as possible the surface water from the part under experiment. The theodolite being stationed as usual over Q, and the vertical wire of the telescope describing a great circle passing through the line QQ1 transverse to the glacier, an assistant (BALMAT), directed by my signals, bored

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* See proofs cited in my Ninth Letter on Glaciers, Edinburgh Philosophical Journal, April 1845, and in second edition of Travels in the Alps, Appendix.

† Mr. WILLIAMSON, Fellow of Clare Hall, Cambridge, to whom I proposed this experimental test of the theory of movement by *echelons*, made a series of independent observations on the Mer de Glace, which coincided in result with what has been stated above. After a patient examination of these facts, and of others which he observed on different glaciers, I am glad to say that Mr. WILLIAMSON was led to abandon the theory of sliding columns or fragments, and to accept that of plasticity as connected with the mechanism of the veined structure which I have endeavoured to illustrate above.

a series of holes from two feet to two feet, forty-five in number, with a common carpenter's centre-bit, and as nearly as possible in the visual line. The holes, which were $\frac{5}{16}$th of an inch in diameter and about five inches deep, were immediately occupied by wooden pins prepared for the purpose. These pins were placed as nearly as possible in the visual straight line, but from the nature of the operation some errors were inevitable. The amount of these errors of position or zero of the marks was immediately determined by causing the vertical wire again to traverse the series, the assistant placing over the centre of the head of each pin in succession the zero point of a scale of inches divided both ways, and held parallel to the length of the glacier, so that (the divisions to tenths of an inch being very plainly marked, and divisible by estimation by the telescope) the fundamental position of each pin was determined, and considered as $+$ if in advance of the transverse line (in the direction of the glacier's motion), and $-$ if behind it (or nearer the origin of the glacier). The mere error of reading did not in any case exceed $\frac{1}{20}$th of an inch, though the uncertainty of centring of the theodolite over **Q** might amount to $\frac{1}{10}$th of an inch, or even more. The two marks nearest **Q** had their positions determined by a thread stretched from the station-pointer of the theodolite to the third mark, their distance being too small to be distinctly seen by the telescope.

The very same process, as regards the placing the zero of the scale on the head of the pin and reading off, was repeated on subsequent days, and the new readings *minus* the fundamental readings gave the *apparent* relative motion in the interval. This apparent motion had to be corrected, exactly as before explained, for the rotation of the visual line due to the translation of the fundamental point **Q**.

The following Table contains—(1.) the original readings on the four days of experiment, namely—

<div align="center">

1844. August 20. 10 A.M.

August 21. 6 P.M.

August 23. 1 P.M.

August 26. 11 A.M.

</div>

(2.) The differences from the fundamental readings or total apparent displacements for each day, reckoning from the commencement. (3.) The same corrected for the rotation of the visual line from the following data :—

August.	Interval. days.	Motion of Q. inches.	Correction at dist. 90 feet. inches.
20 to 21.	1·33	16	0·56
20 to 23.	3·12	38	1·33
20 to 26.	6·08	73	2·56

Inches

48
44
40
36
32
28
24
20
16
12
8
4
0

Q   10 Feet   20   30   40
                                      Obs.

Inches
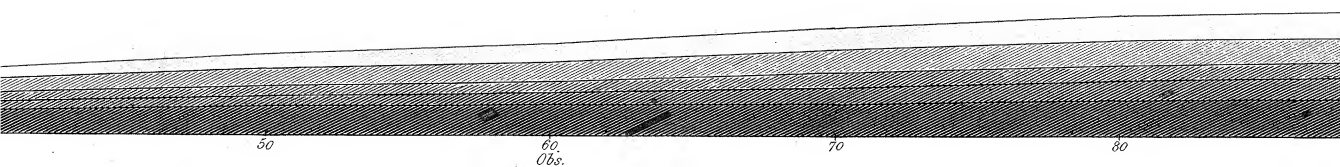
15
10
5
0

Q   1   2   3   4   5   6   7   8   9   10   11

23. 22.
21.

Aug. 26.
23
21

Fig. 1. Diagram of the Flexure (in a horizontal Plan
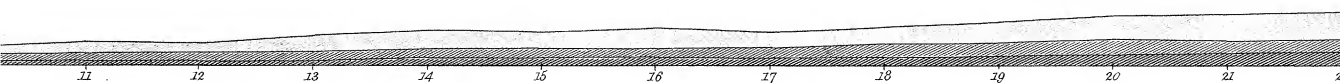*at Station Q, from the 9.ᵗʰ to the 26.ᵗʰ August. From o*

Horizontal Scale
Vertical Scale (Co

50 60 70 80
Obs.

Fig. 2. Diagram of the Flexure of a Tran
at Station Q, from the 20.ᵗʰ to the 25.ᵗ

Horizontal Scale $\frac{1}{40}$ of Nature. — V

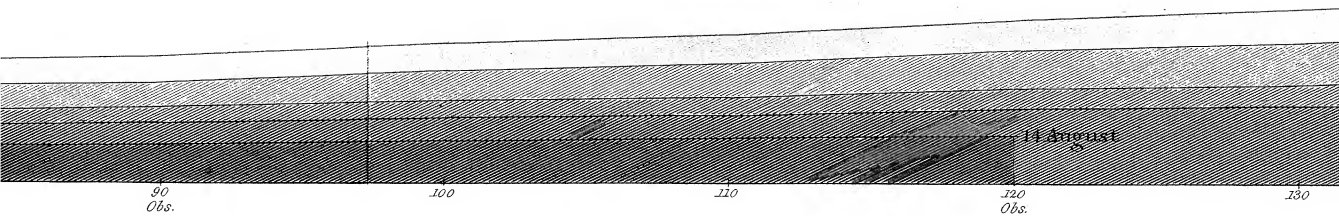11 12 13 14 15 16 17 18 19 20 21 2

Plane) of a Transverse Space of the Mer de Glace, 180 feet in length
rom observations made at 6 points viz.ᵗ at 0, 30, 60, 90, 120 & 180 feet.

l Scale ...... $\frac{1}{80}$ *of Nature*
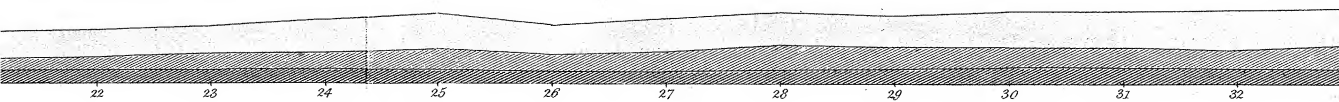*cale (Corresponds to forward Motion)* $\frac{1}{40}$

11 August

90
Obs.   100   110   120   130
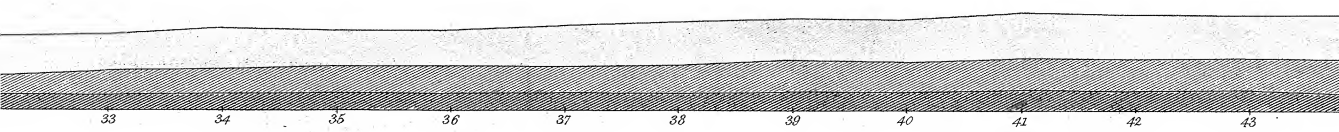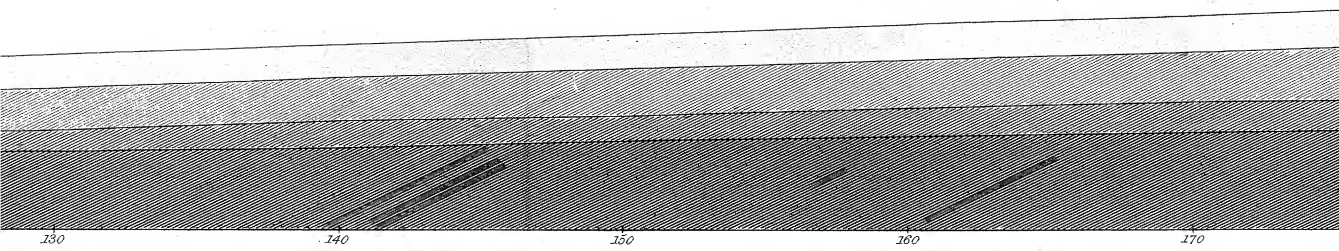   Obs.

a Transverse Space of the Mer de Glace 90 feet in length
the 25ᵗʰ August 1844, observed at intervals of 2 feet.

ure. — Vertical Scale (Corresponds to forward Motion) $\frac{1}{20}$

22   23   24   25   26   27   28   29   30   31   32

slight

slight.

26 Aug.ᵗ

23

19

17

9 Aug.ᵗ
180 Feet.
Obs.

Aug: 26.

Aug: 23.
Aug: 21.
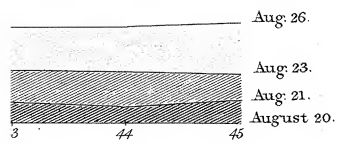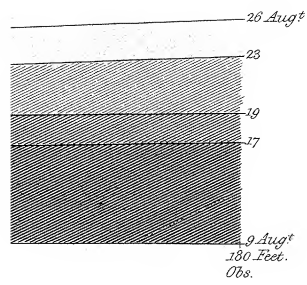August 20.

3   44   45

TABLE IV.—Showing the Apparent and True relative motions of forty-five points two feet apart, in a line transverse to the axis of the Mer de Glace, 1844.

Mark, No.	Readings of Position.				Apparent Motion.			Corrected Motion.			Remarks.
	Aug. 20, 10 A.M.	Aug. 21, 6 P.M.	Aug. 23, 1 P.M.	Aug. 26, 11 AM.	August 20, 21.	August 20—23.	August 20—26.	August 20, 21.	August 20—23.	August 20—26.	
1.	+0·15	−0·1	−0·25	0·0	−0·25	−0·4	− 0·15	−0·25	−0·45	−0·15	Between 1 and 2 a slight fissure.
2.	+0·3	−0·5	−0·25?	− 0·2	−0·8	−0·55	− 0·5	−0·8	−0·6	−0·55	
3.	+0·15	+0·15	+0·6	+ 0·7	0·0	+0·45	+ 0·55	−0·05	+0·35	+0·4	
4.	−0·2	−0·1	+0·25	+ 0·3	+0·1	+0·45	+ 0·5	+0·05	+0·35	+0·3	
5.	−0·25	−0·35	−0·05	+ 0·3	−0·1	+0·2	+ 0·55	−0·15	+0·05	+0·25	
6.	−0·25	0·0	+0·6	+ 1·2	+0·25	+0·85	+ 1·45	+0·2	+0·65	+1·1	Veined structure very strong.
7.	−0·2	+0·2	+0·9	+ 1·8	+0·4	+1·1	+ 2·0	+0·3	+0·9	+1·6	
8.	0·0	+0·3	+1·2	+ 2·45	+0·3	+1·2	+ 2·45	+0·2	+0·95	+2·0	Between 9 and 10 a fissure.
9.	0·0	+0·5	+1·45	+ 2·5	+0·5	+1·45	+ 2·5	+0·4	+1·2	+2·0	
10.	−0·2	+0·15	+1·1	+ 2·3	+0·35	+1·3	+ 2·5	+0·25	+1·0	+1·95	
11.	−0·4	+0·25	+1·35	+ 2·7	+0·65	+1·75	+ 3·1	+0·5	+1·4	+2·45	
12.	−0·0	+0·5	+1·6	+ 2·85	+0·5	+1·6	+ 2·85	+0·35	+1·25	+2·15	
13.	−0·55	+0·15	+1·45	+ 3·3	+0·7	+2·0	+ 3·85	+0·55	+1·6	+3·1	
14.	−0·55	+0·35	+1·5	+ 3·7	+0·9	+2·05	+ 4·25	+0·75	+1·65	+3·45	
15.	−0·15	+0·6	+2·05	+ 4·15	+0·75	+2·2	+ 4·3	+0·55	+1·75	+3·45	
16.	−0·2	+0·65	+2·2	+ 4·5	+0·85	+2·4	+ 4·7	+0·65	+1·9	+3·8	
17.	−0·25	+0·6	+2·2	+ 4·15	+0·85	+2·45	+ 4·4	+0·65	+1·95	+3·45	
18.	−0·15	+0·7	+2·4	+ 4·85	+0·85	+2·55	+ 5·0	+0·65	+2·0	+4·0	
19.	−0·1	+0·95	+2·5	+ 5·35	+1·05	+2·6	+ 5·45	+0·8	+2·05	+4·4	
20.	−0·15	+1·0	+3·0	+ 5·9	+1·15	+3·15	+ 6·05	+0·9	+2·55	+4·9	Two fissures between 19–20 and 20–21.
21.	+0·25	+1·7	+3·55	+ 6·7	+1·45	+3·3	+ 6·45	+1·2	+2·7	+5·25	
22.	+0·1	+1·6	+3·5	+ 6·7	+1·5	+3·4	+ 6·6	+1·25	+2·75	+5·35	
23.	+0·3	+1·7	+4·0	+ 7·55	+1·4	+3·7	+ 7·25	+1·1	+3·0	+5·95	22–23, slight fissure very oblique.
24.	+0·2	+1·65	+4·0	+ 7·9	+1·45	+3·8	+ 7·7	+1·15	+3·1	+6·35	
25.	−0·3	+1·15	+3·5	+ 8·0	+1·45	+3·8	+ 8·3	+1·15	+3·05	+6·9	
26.	+0·35	+1·7	+4·3	+ 7·95	+1·35	+3·95	+ 7·6	+1·05	+3·2	+6·1	26–27, a slight fissure. It recrosses the line at 40–41.
27.	+0·25	+1·75	+4·4	+ 8·45	+1·5	+4·15	+ 8·2	+1·2	+3·35	+6·65	
28.	+0·25	+2·05	+4·9	+ 9·1	+1·8	+4·65	+ 8·85	+1·45	+3·8	+7·25	
29.	+0·4	+2·2	+5·15	+ 9·35	+1·8	+4·75	+ 8·95	+1·45	+3·9	+7·3	
30.	+0·15	+1·9	+4·9	+ 9·3	+1·75	+4·75	+ 9·15	+1·4	+3·85	+7·45	
31.	0·0	+2·0	+4·7	+ 9·0	+2·0	+4·7	+ 9·0	+1·65	+3·8	+7·25	
32.	+0·2	+2·3	+5·3	+ 9·7	+2·1	+5·1	+ 9·5	+1·7	+4·15	+7·7	
33.	+0·2	+2·1	+5·5	+10·0	+1·9	+5·3	+ 9·8	+1·5	+4·3	+7·95	
34.	+0·9	+2·9	+6·0	+10·95	+2·0	+5·1	+10·05	+1·6	+4·1	+8·15	
35.	+0·25	+2·2	+5·7	+10·5	+1·95	+5·45	+10·25	+1·55	+4·4	+8·25	34–35, a very slight fissure.
36.	+0·1	+2·1	+5·5	+10·15	+2·0	+5·4	+10·05	+1·55	+4·35	+8·0	
37.	−0·1	+2·05	+5·3	+10·5	+2·15	+5·4	+10·6	+1·7	+4·3	+8·5	
38.	0·0	+2·1	+5·9	+11·3	+2·1	+5·9	+11·3	+1·65	+4·75	+9·15	
39.	+0·2	+2·4	+6·35	+11·8	+2·2	+6·15	+11·6	+1·75	+5·0	+9·4	
40.	0·0	+2·3	+5·9	+11·1	+2·3	+5·9	+11·1	+1·8	+4·7	+8·85	
41.	+0·1	+2·5	+6·4	+12·15	+2·4	+6·3	+12·05	+1·9	+5·1	+9·7	40–41. See 26–27.
42.	0·0	+2·3	+6·25	+11·8	+2·3	+6·25	+11·8	+1·8	+5·0	+9·4	
43.	−0·1	+2·2	+6·15	+11·65	+2·3	+6·25	+11·75	+1·8	+4·95	+9·3	
44.	0·0	+2·4	+6·5	+12·35	+2·4	+6·5	+12·35	+1·85	+5·2	+9·85	
45.	−0·2	+2·4	+6·1	+12·25	+2·6	+6·3	+12·45	+2·05	+4·95	+9·9	

The results of these observations will be best understood by consulting the projected results in Plate IX. fig. 2, in which the advance of the marks is magnified twofold relatively to the spaces between them, which was necessary for the reduced scale of the engraving, although it has the disadvantage of increasing the deviations from symmetry which might arise from errors of observation. As those who have not attempted the actual execution of such experiments cannot be aware of the difficulties they entail, it may be just to mention that during weather so unfavourable as that which occurred

during the continuance of these experiments, nothing can be so irksome as the necessity of persevering in the face of physical obstacles, the only alternative which the necessary limitation of my stay afforded being to abandon them. I do not speak of the painful effort of conducting delicate observations for hours under a hot sun, whilst the feet are immersed in the liquid sludge of decaying snow, for I am not aware of having sacrificed the precision of a single observation to such a cause (though in the course of my glacier experience I have sometimes been compelled to abandon or discontinue observations), but it is easy to see that the success of experiments like this depended upon the absolute fixity of the marks inserted in the unstable and wasting surface of the glacier, and that the most dry and uniform condition of the ice seemed alone to promise a chance of finding the small pins in the exact positions in which they had been planted a day or two before. Instead of this, eleven out of nineteen days which I spent at Chamouni were wet, and notwithstanding the season of the year, the glacier was repeatedly covered with snow, which in melting under a succeeding fierce sun, left the surface honeycombed by infiltration and streaming with wet, so that the preservation of the holes was only effected by laboriously covering every one with large flat stones during the intervals of observation, and even this was not free from other disadvantages which it would take too long to particularize. On the whole, the uniformity of the triple curves exhibited in fig. 2 is surprising, considering the local errors to which the fixation of the pins was liable, and the smallness of the quantities sought. The first two curves, those for the 21st and 23rd August, are indeed as perfectly regular as it would be possible to expect from this kind of observation, even much more so than I had ever hoped to attain; but on the 26th the holes containing the pins were more degraded, and some manifest errors have arisen from this cause, and evidently affect only single marks, such as the twelfth and twenty-fifth mark, which singly have inclined forward or backward by the fusion of the ice. With these preliminaries as to the reasons why the irregularities of these curves should be judged with indulgence, I will state briefly the most apparent general results.

First. The flexure of the ice is proportional, almost exactly to the time elapsed in the intervals of the observations; and it is also graduated from point to point, not *staccato,* as would inevitably have been the case had the relative motion been due to the sliding of finite portions past one another, as in Plate VII. fig. 3. We perceive nothing of the kind.

Secondly. A singular peculiarity strikes the eye, which at first puzzled me, but when the cause was explained, confirms in no slight degree the accuracy of the methods employed and their fitness to reveal the minutest motion of the glacier. The curves of Plate IX. fig. 2, cut the axis, not all exactly at the same point; but on an average this point may be fixed with tolerable accuracy between the third and fourth mark, or seven feet from the fundamental station Q. *The first and second marks moved evidently slower than the point Q, or the zero,* and the progress of the third and fourth is dubious or irregular. The cause of this peculiarity was clearly ascer-

tained *on the spot* to be the existence of two crevasses belonging to the lateral system of crevasses, between which at their thinning out the station Q had been placed, under the idea that their distance was sufficiently great not to affect the motion, The position of these crevasses is shown in Plate VIII. fig. 3, by which it will be seen that they were fifty feet apart in the direction of the length of the glacier, and that a line joining their extremities passed eight feet nearer the centre of the glacier than the point Q, thus almost coinciding with the point of contrary flexure, which was evidently occasioned by a slightly superior advance of the mass of ice on which Q was placed, thus insulated in some degree between the two fissures. This enables us to transfer the origin of our curve to a point of undoubted solidity, namely, the fourth station, from which point it swells with the regularity which has been described, and which establishes the compactness of the ice experimented on in the most convincing manner.

Thirdly. The curves reckoned from the origin, thus experimentally indicated, show in a beautiful manner the convexity in the direction of the glacier motion before alluded to, which is singularly striking, considering the shortness of the space in which it is developed with nearly mathematical precision, being only about $\frac{1}{25}$th of the breadth of the glacier in this place (see ground plan, Plate VIII. fig. 1.). Even an inspection of the curves (Plate IX. fig. 2) can faintly convey the impression made upon my own mind, when upon the 26th of August I placed the theodolite for the last time over station Q, and caused the vertical wire to pass in front of the line of pins bent into the convex shape by the relative motion of six days' continuance. Thus seen in foreshortened perspective, the eye would in an instant have seized an abrupt motion or discontinuity of the line, but " the appearance of the curve they formed was beautiful ; the whole line of pins was deviated from the usual line QQ1 by an angle equal to 12·45 inches, seen at a distance of ninety feet, or about 40', and besides this, the pins lay in a beautiful and nearly continuous curve, presenting its convexity towards the valley, and decidedly without any great step or start. This was beautifully seen when I directed the vertical wire of the theodolite upon the forty-fifth pin and caused it to describe a vertical plane*. I observed however a curious fact, plainly indicated by the numerical results ; the curve crossed the axis at the fourth pin, and attains its greatest convexity at the twenty-fifth†."

Fourthly. That no information might be wanting as to the precise condition of the mass of ice under experiment, I made a very minute examination of the state of the transverse line with respect to the occurrence of *flaws* in the ice. The most important of these was one which returned into itself, crossing the line towards the origin of the glacier between the twenty-sixth and twenty-seventh marks, and returning backwards between the fortieth and forty-first without extending further upwards. Such a flaw, even if devoid of cohesion, could only act by allowing the piece of ice contained between the twenty-seventh and fortieth mark to slip bodily forwards,

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* An approximation to this effect will be obtained by stretching a fine thread over the figure.

† From my notes made at the time.

leaving a vacuity behind. No such slip is recorded in the observations, and no such gap or vacuity was left during the continuance of the observations for seventeen days. Nothing approaching to an open fissure occurred in any part of the space of ice under observation, and the few flaws noted (see the column of remarks in Table IV., and the indications of them by dotted lines and arrows in Plate IX. fig. 2, showing their directions) were perfectly close, and all more or less of zigzag forms, preventing the possibility of sliding. The *veined structure* was developed in every part of the section, in some parts more admirably than others, as near the seventh mark, and between the fortieth and forty-fifth. These countless blue veins may be considered as so many flaws or partial solutions of continuity, existing or having existed; they are almost perfectly *straight*, and (as will be shown immediately) *exactly* in the direction in which the relative motion of the parts of the ice demonstrated by these experiments takes place. It would require strong evidence to convince us that these veins are not occasioned by, and the mechanism of, the plastic motion of the ice.

The beautiful convexity of the curves in the direction of motion which the eye at once seized, and could more accurately distinguish by the theodolite, as having its largest sagitta at the twenty-fifth mark, namely, almost exactly midway between the fourth mark or convex origin, and the forty-fifth or extreme mark, contains in itself an evidence with which no person of correct habits of thought can fail to be struck as proving a regular plastic action of gravity or other propelling force acting from point to point on the mass of the glacier. I made however a check experiment almost unnecessary, but which I will here detail. Lest it should be alleged that the whole area under experiment was a moving one capable of being *swung* round by the pressure upon the centre of the glacier, so that the displacement of the transverse line was due to rotation of the mass operated on round some distant centre, I took care near the commencement of my experiment to fix a mark in the ice in the same line with Q parallel to the length of the glacier, or perpendicular to the transverse visual line at the commencement of the experiment. This point is marked q in Plate VIII. fig. 3. Now had the block of ice on which the marks Q, q, (1), (2), (3), &c. were fixed, been revolving round some fixed or moveable centre, the right angle (3) Qq would have remained a right angle, and so for (2) Qq and (1) Qq. But these angles constantly became more obtuse, and that in different degrees depending on the convexity of the curve Q, (1), (2), (3) : and so of (4) and (5). It is impossible, therefore, to avoid the conclusion, that the solid ice was itself *distorted* to the amount of the excess of progression of the more central above the lateral stations.

The results are contained in the following Table, the five stations being those formerly mentioned at 30, 60, 90, 120, and 180 feet from Q. The first three stations were fixed when the visual line had an azimuth of 89° 55′ from q; the fourth was fixed on the 14th of August, when the visual line had revolved through 3′ (its daily progression towards q in consequence of the translation of the station Q being 1′ 30″ exactly), and the fifth on the 17th, when the visual line was in azimuth 89° 47½′.

TABLE V.—Showing the variable azimuths (observed) of the transverse stations with the longitudinal direction Q*q*.

Date.	(1).	(2).	(3).	(4).	(5).	Angle Q1 Q *q*.
August 12. 2 P.M.	89 55	89 55	89 55	. . . . . .	. . . . . .	89 55
14. 1 P.M.	90 6½	90 5	90 5	89 52		
17. Noon.	90 26	90 24	90 23	90 8	89 47½	
19. 5 P.M.	90 47	90 46	90 44	90 10	89 52	
21. 6 P.M.	90 56	90 56	90 53	90 21	90 3	
23. 1 P.M.	91 9½	91 9½	91 4½	90 34	90 14	
26. Noon.	91 24	91 23	91 18	90 45	90 24½	89 28½

In the last column is the observed azimuth of the visual line joining the fixed point Q1 with the moveable station Q, from the mark *q* in the glacier, on the first and last observation. This angle had therefore diminished by 26′½. Now from the known progression of Q, and the known distance QQ1, we computed at page 166 the rotation of the visual line, and found it to be 25′ 54″. This close correspondence is highly satisfactory, as showing that the relative positions of Q and *q* remained unchanged during the continuance of the experiment with reference to the imaginary transverse line by which they were adjusted. It is worthy of notice that the line Q*q* was exactly in the direction of the veined structure.

Edinburgh, July 1845.

Fig.6 Diagram of the Flexure in a horizontal Plane of a Dimensive Space of the Mer de Glace that has a length at Station if from the P to the 10th Square. From observations made in 2 points 145 et 242 feet apart

Fig.7 Diagram of the Flexure of a Dimensive Space of the Mer de Glace 92 feet in length at Station if from the 10th to the 11th Square 98 observed at intervals of 2 feet